Westlaw
8 ELECTLJ 85
8 Election L.J. 85

Election Law Journal
2009

Original Article

*85 MODELING PROBLEMS IN THE VOTER IDENTIFICATION—VOTER TURNOUT DEBATE

Robert S. **Erikson**, Lorraine C. Minnite [FNa1]

IN APRIL 2008, the U.S. Supreme Court upheld Indiana's controversial voter identification (ID) law. Adopted in 2005, the law requires voters to show a current, government-issued photo identification. Opponents worry voter identification rules will place an undue burden on the voting rights of elderly, low income, and minority voters, disputing the need for the rules. Nevertheless, over the last five years, stricter voter identification requirements have been adopted on party line votes in more than a dozen states. Stimulated by the pressing policy debate, recent scientific research on the turnout question suggests that the most stringent rules will have harmful effects. However, the complexity of electoral laws and voting behavior together with the likely marginal effect of photo ID rules makes statistical outcomes quite sensitive to research designs. We see problems with existing designs that rely on individual, self-reported voting records from the Current Population Survey. Our article evaluates this research and disputes the strength of the statistical arguments used to support findings of an observable negative effect on turnout from voter ID laws. Alternatively, we adjust the models using state samples and difference-indifferences techniques and reanalyze the CPS data for the 2002 and 2006 midterm elections. While we do not conclude that voter ID rules have no effect on turnout, our data and tools are not up to the task of making a compelling statistical argument for an effect.

INTRODUCTION

In a widely reported story from the 2008 presidential primary in Indiana, twelve elderly nuns were turned away from their resident convent polling place by a fellow sister because they failed to comply with the state's new voter identification rules (Hastings 2008a; 2008b; Gordon 2008; Martelle 2008). The week before, the Supreme Court had upheld Indiana's controversial law which compels citizens to show a current government-issued photo ID in order to vote. [FN1] As voter registration surged in anticipation of a hotly contested primary (Jacobs and Burns 2008; “Voter Registration Numbers” 2008), voting rights advocates worried that new or vulnerable voters would not be able to vote because of failure to present the appropriate ID. In the end, however, despite record turnout, there were few official reports of vote denial in Indiana (Indiana Secretary of State 2008), leading defenders of stricter voter ID laws to feel vindicated (Hastings 2008c). Important questions, however, remain. They arise from concerns like those expressed by the *(Muncie, IN) Star Press* three days after the primary:

> While only 20 provisional ballots were cast in Tuesday's election—and not all of ***86** them because of a lack of ID—it is unknown how many were turned away from the polls by inexperienced [poll] workers, but there is anecdotal evidence it happened. [One disabled] veteran, for example, wasn't given a provisional ballot in Precinct 23 until a mob of voters outside demanded it, going so far as to ask a Democratic party official to come to the polling place. (“Indiana Voter ID Law Disenfranchised Some” 2008)

Our vignette from the Hoosier State presents a puzzle for courts that may hear future voter ID disputes and for the social science upon which lawyers, judges, and advocates in voting rights cases often rely. Do voter ID laws deter voting? Do the data and instruments we have allow us to detect marginal influences on voting stemming from a single voting rule? Courts need to

know in order to better evaluate the nature of the burden the rules may impose on the right to vote.

The problem is the silence in the available data. Until the current controversy, there was little scientific analysis of the relationship between documentary ID rules and voting, and for good reason: six years ago only 11 states required all voters to present documentary proof of their identity at the polls before casting a ballot (Electionline.org 2006). That number has since more than doubled to 24 (Project Vote 2007). At the same time, while these laws are rhetorically defended as anti-fraud, voter confidence, "good government" reforms, none of the legislative sponsors of voter ID bills have made any credible showing of voter fraud to justify the need for more ballot security. [FN2]

We could generously conclude that politicians have tightened voter ID laws on the faith that they are, as Indiana elections officials put it, only "a party-neutral, good-government reform ..." (Brief of State Respondents 2007, 37). But the politics surrounding the statehouse slugfests over the voter ID issue suggest something else. Politicians clearly see this issue through the lens of party politics and electoral advantage. Few other issues are as politically polarizing. For example, 95.3 percent of 1,222 Republican legislators but just 2.1 percent of 796 Democrats voting on ten voter ID bills introduced by Republican state legislators between 2005 and 2007 supported them. (Brief of Amici Curiae 2007, 28). Given the long history of partisan maneuvers to win elections by excluding certain voters under the guise of "good government" reform (Kousser 1974; Piven and Cloward 2000), the effects of voter ID laws on voting deserve serious scientific scrutiny. In the absence of evidence, the perception of a party advantage in tightening up voter ID requirements is driving the debate.

Are the data and instruments we have up to the task of finding what may be a needle—e.g., 12 elderly nuns in South Bend, Indiana—in a haystack? Researchers analyzing whether voter ID laws influence turnout have approached the question in three ways. Several studies construct statistical models to test for relationships between the degree of burden imposed by voter ID requirements and voter turnout levels, looking for any disproportionate effects among different groups of voters (Lott 2006; Eagleton Institute 2006; Vercellotti and Anderson 2006; Mulhausen and Sikich 2007; Mycoff, Wagner and Wilson 2007; Alvarez, Bailey and Katz 2008; Milyo 2007; Logan and Darrah 2008). Others conduct surveys or match government lists to estimate the proportion of the electorate lacking the requisite ID and to examine whether patterns in the possession of ID vary among groups (Brace 2005; Pawasarat 2005; Brennan Center 2006; Barreto, Nuño and Sanchez 2007a; 2007b; Hood and Bullock 2008). A third approach, using survey data to assess attitudes among voters toward stricter voter ID, tests two different assumptions. One concerns the strength of public support for voter ID as a rationale supporting these laws (finding high levels of support, generally; see, for example, Pastor, et al. 2008). The other frames voter ID laws as at least a partial remedy for a lack of confidence in electoral administration, hypothesizing that as public confidence increases so, too, will turnout (finding little support linking perceptions about the frequency of ***87** voter fraud to a lack of confidence in electoral administration, or to turnout; see Ansolabehere and Persily 2008).

THE CURRENT POPULATION SURVEYS AND ESTIMATING THE EFFECTS OF VOTER IDENTIFICATION LAW

Our article is concerned with the first approach to the question of voter ID laws and turnout effects, specifically with statistical models using Current Population Survey (CPS) data to measure turnout. Given the wealth of information it provides regarding voter participation, the best data source would seem to be the U.S. Census's post-election turnout surveys—the Current Population Survey's Voter Supplements collected every other November. Approximately two weeks after a national election, CPS respondents are asked whether they voted and, if not, whether they are registered. Even when limited to respondents who claim to be registered, the CPS provides tens of thousands of survey responses to work with every two years.

At least three influential (though unpublished) studies have examined potential vote suppression using CPS data (Vercellotti and Anderson 2006; Mulhausen and Sikich 2007; Alvarez, Bailey, and Katz 2008). In each case the authors conducted multivariate probit or logit analyses of voting amongst registrants as a function of a host of relevant individual characteristics plus a measure of the state laws governing voter identification. The results are somewhat contradictory.

One study, commissioned by the U.S. Elections Assistance Commission (EAC), was performed by the Eagleton Institute of

Politics at Rutgers University and the Moritz College of Law at Ohio State University (Eagleton Institute of Politics 2006; Vercellotti and Anderson 2006). Vercellotti and Anderson explored statistical relationships between the stringency of voter ID laws and turnout in the 2004 presidential election. Controlling for demographic variables (i.e., age, race, education, and income) and political context (i.e., a competitive election), factors known to influence voter turnout, the authors found seemingly compelling statistical evidence of a negative causal relationship between the stringency of a state's voter ID requirements and voter turnout, with the greatest suppressive effect among racial minorities, especially Latinos. Vercellotti and Anderson's findings were challenged, however, in a paper by Muhlhausen and Sikich (2007) of the Heritage Foundation. Once Muhlhausen and Sikich made what they contend are corrections and improvements to the models, the statistical significance of the negative relationship found by Vercellotti and Anderson between ID stringency and turnout in the individual level data largely disappeared.

Alvarez, Bailey, and Katz (2008) offer the most statistically sophisticated treatment of the voter ID-voter turnout modeling problem to date, employing a Bayesian multi-level model to examine turnout in the CPS individual-level data for the four federal elections held between 2000 and 2006. They make useful refinements to the measurement of state voter ID laws, generating an eight-level index of severity. As with Vercellotti and Anderson, they find statistical evidence of a slight relationship between the restrictiveness of voter identification laws and turnout. However they do not find the effects to be strongest among racial minorities.

These papers' findings are sometimes inconsistent, not only across studies but also (sometimes) within the same study. Given the limited size of the effects that are searched for, small changes in choices such as how to measure the independent variables and which controls to impose can alter the conclusions. We therefore address in this article some fundamental issues of research design and statistical inference. Initially, we question whether cross-sectional analysis of CPS data (e.g., of the 2004 election only) is appropriate. Suppose, for instance, that unmeasured causes of state turnout levels (e.g., “culture”) affect the states' propensity to pass severe voter identification laws to even a slight degree. This causal process could distort the evidence regarding the small effect, if any, of identification laws on turnout.

This problem is compounded by possible pitfalls in the interpretation of a multilevel model involving state-level causal variables and individual data. While controlling for individual-level variables helps achieve statistical precision,***88** it is also necessary to statistically treat the independent variable of interest or treatment effect—state voter identification policy—as an aggregate state level variable. This means that when reporting coefficients involving voter identification laws, the studies should report clustered standard errors. The problem is that the large *N* of over 64,000 cases (in the 2004 analysis) provides the illusion of more statistical power than is present. Although the individual-level variables provide some controls, with only 50 states plus D.C., the effective *N* for calculating standard errors from the individual-level data is merely 51. Only if it were possible to control for all state-level variables affecting voter turnout would clustering cease to be a problem.

Despite frequent discussion in the econometric and statistical literature (e.g., Moulton 1986, 1990; Wooldridge 2003; Donald and Lang 2007), the need to impose clustered standard errors is not always appreciated by practitioners. (For a political science example applied to state legislation, see Branton 2004, and Primo, Jacobsmeier, and Milyo 2007; for an accessible general discussion of clustered standard errors, see Rogers 1993.) Failing to impose clustered standard errors results in the reporting of false positives—findings reported as statistically significant when the proper (larger) standard error would show that they are not. When trying to find small effects of voter identification laws in the states using the CPS Voter Supplement survey data, the danger is that the presence of thousands of individual data points offers a false sense of certainty.

None of the three voter ID studies cited above reports the appropriate clustered standard errors. Both the Vercellotti and Anderson and the Muhlhausen and Sikich studies report using “robust” standard errors. But (as we will show below) this does not properly address the problem at hand. The Alvarez et al. method for reporting their confidence intervals is not fully transparent from their report. Clearly, however, the standard errors reported for state-level variables are smaller than is appropriate. We know this because the reported standard errors (or confidence intervals) are equally small (if not smaller) for dichotomous state-level variables as they are for individual-level dichotomous variables. This should not be. The effective *N* for state level variables is 51. For individuals, the effective *N* is in the tens of thousands.

THE CHALLENGE

We return to the questions at hand. Do voter ID laws suppress turnout? Is their effect particularly severe among certain disadvantaged groups whose erasure from the electorate could tilt the partisan outcome? As social scientists can we document the effect from analyzing the usual turnout data, such as from the CPS?

Let us accept, at least for heuristic purposes, the first two claims, while stipulating that the effects must be small, consistent with some of the research reviewed above. For the sake of argument let us pull some numbers out of the hat as generous conjectures about the short-term effects of a draconian voter ID law. First, assume that when a state goes from no ID required to the demand for a government-issued photo ID, the requirement prevents two percent of the registered electorate from voting. Of this two percent, three out of four would have voted if allowed, which (we assume) is the same rate as those with the required photo IDs. Thus, of the original electorate, 98 percent show up to vote displaying their IDs, while two percent either are intimidated by the law to stay home or are refused when they show up at the polls. Let us also assume that if they could vote, our newly disenfranchised voters would split one-sidedly as 80 percent Democratic versus 20 percent Republican. Before disfranchisement, our missing two percent would add .02 x .80 to the Democratic vote or .016. This is .06 above what they would have contributed if they split a neutral 50-50. Now, if, say, the 98 percent with their photo IDs split as evenly as 49.5 percent Democratic and 50.5 percent Republican, our missing voters could make the difference if they voted ((.98 x .495) + (.02 x 80) = .4851 + .016 = .5011).

If these numbers are approximations of what politicians believe, then on partisan grounds alone, the battle is worth waging. (In effect, our hypothetical numbers would mean that the decisive partisan threshold for the Democratic ***89** party goes from 50 percent to 50.5 percent of the two-party vote.) Given our fake numbers, many would see a normative imperative as well, with facilitation of the exercise of the right to vote outweighing the possible phantom of voter fraud.

But our question here is different. If two percent of the eligible electorate go missing due to voter-ID disfranchisement, are our instruments truly capable of detecting it? In asking this question we must be wary not only of false negatives (as when researchers claim they find evidence that ID laws have no effect) but also of false positives (as when researchers claim they find convincing evidence that voter ID laws do matter).

Here, we analyze the CPS data, using the basic technique of difference-in-differences, in which we ask whether the change from 2002 to 2006 in our dependent variable (turnout among registered voters) varies as a function of the change in our treatment variable (the presence or absence of new voter ID laws enacted between the 2002 and 2006 elections). For possible controls we have the characteristics of the individual voters in the CPS survey. For units, the appropriate level is the set of 50 states plus the District of Columbia. Thus, while using a survey with multiple thousands of respondents, we collapse the data into 51 large state samples.

We do not claim that our methodology is the only one worthy for this task or even the best. But it does illustrate how the task of estimating the effects of voter ID laws is truly daunting. The handicaps are obvious. We start with the expectation that any effect is small as we search for a possible missing two percent of the registered electorate. And even though we can observe treatments in the form of new voter ID laws enacted between 2002 and 2006, these are mostly mild innovations, usually falling short of requiring photo IDs. Therefore, the expected effect is even smaller. In addition, we have the handicaps that come with working with voter surveys. Although this tendency may be minimal in the context of the non-political CPS survey, people do lie to pollsters, exaggerating their voting histories. Perhaps the biggest hurdle of all, we must ask whether the undocumented voters who are otherwise eligible and registered are fully represented in even well-run Census surveys.

Finally, despite the fact that CPS surveys include thousands of respondents, the effective quantity of cases is not the number of survey respondents but the number of states that generate the treatments by changing or not changing their voter ID policies. This is a central lesson of this article. Now, having listed the arguments against finding anything, let us turn to the data.

RESEARCH DESIGN

We estimate the possible effects of voter ID laws by means of a difference-in-differences test applied to 2002 and 2006 voter participation data. Difference-in-differences analysis simply is the current econometric term for comparing the degree of change for different treatment groups (Bertrand, Duflo, and Mullainathan, 2004). Some will recognize the method as Campbell and Stanley's (1966) “non-equivalent control group design.” Specifically, with states as the units, we ask: did state-level voter participation change between these two midterm elections as a result of changes in the states' voter ID legislation? The idea is simple. The independent variable is change in legislation between the two elections. The dependent variable is change in voter participation among registered voters between the same two elections. If voter ID laws suppress turnout, the relationship should be negative: increased voter ID requirements should be associated with lower voting rates.

Especially in a non-experimental setting, it is helpful to control for additional sources of variation in the dependent variable. The more controls, the less the concern about spurious relationships. And the more the extraneous sources of variance are controlled, the more similar are the treatment groups apart from the independent variable of interest. Limiting the unexplained variance enhances the statistical power of the comparisons across treatment groups. With group level treatments, it is important to take into account the clustering of the group level effects. Although the likelihood of finding a statistically significant result is greater ***90** when there is a large number of “degrees of freedom,” the appropriate degrees of freedom for estimating the standard error of the group treatment effect is the number of groups, not the number of subjects (e.g., potential voters) across groups. At the same time, gains can be made by controlling for individual characteristics (such as the demographic traits of CPS respondents). [FN3]

Our goal is to tell a cautionary tale, illustrating the limitations of our statistical enterprise. We believe our method of statistical modeling is subject to little bias and approaches the limit in how much information can be reliably wrung from the data. Nevertheless, the errors in our estimates are inherently large, so that the search for small effects of voter registration legislation must be inconclusive. It follows that one cannot yet say much about the effect of voter ID laws from studying voting participation data in the states.

Our study measures voter participation in 2002 and 2006 as the participation rate of registered voters among each state's sample in the CPS November Voting and Registration Supplements. With over 64,000 registered voters in each survey, the CPS provides state estimates based on more than 1,000 respondents per state. We use the CPS rather than official turnout numbers because of concerns about un-even purging of the registration rolls in the state. Whereas turnout as a percentage of the theoretically eligible is readily available from official sources at the state level (subject to some concerns about who should be included in the eligible voter denominator), the turnout rate as a function of official registration figures is more problematic.

A second reason for using the CPS is that the CPS survey offers controls for some individual characteristics of state electorates. Vercellotti and Anderson (2006), Muhlhausen and Sikich (2007), and Alvarez et al. (2008), model respondents as the unit; we see states as the proper unit, while still using individual-level analysis to adjust state estimates.

Our measure of legislation is the ordering of eight types of requirements for voting at the polls. Borrowed from Alvarez et al. (2008), these are, in order of increasing stringency:

0. Voter must state his/her name
1. Voter must sign his/her name in a poll book
2. Voter must sign his/her name in a poll book and it must match a signature on file
3. Voter is requested to present proof of ID or voter registration card
4. Voter must present proof of ID or voter registration card
5. Voter must present proof of ID and his/her signature must match the signature on the ID provided
6. Voter is requested to present photo ID
7. Voter is required to present photo ID.

There are further variations, and some increments may be more severe than others. Only two states had gone to level 7 by 2006. One, Indiana, required a government-issued photo ID while the other, Florida, was less strict about the source. In our

analysis we measure change either as the net change in the numerical value (0-7) or the presence or absence of an increase in severity. When perusing details of the data, we keep a special eye on the two "7" states, Florida and Indiana.

The setup then is a bivariate analysis for 50 state observations. We perform OLS regression equations where the dependent variable is change in turnout. The independent variable is the change in voter identification legislation, either as the change score on the 0-7 scale or the presence or absence of change.

The main measure of voter participation is the observed voting rate among CPS registrants. We supplement this with an adjusted (residual) rate as the mean state rate controlling for a set of individual-level characteristics of the respondent—age, education, income, race, gender, and marital status. These controls (constructed similarly but not exactly as here) play a central role in Alvarez et al.'s (2008) individual-level analysis. Our state-level dataset is displayed in the Appendix.

As a baseline for turnout levels we use the set of individual-level logit equations shown in the first set of columns of Table 1 (labeled ***91** "Without Voter ID Laws"). Each respondent obtains a predicted turnout probability based on these equations. The adjusted state turnout level (or residual) then is the deviation of the observed turnout in the state sample from that predicted by demographic characteristics.

TABLE 1. CROSS-SECTIONAL LOGIT EQUATIONS PREDICTING VOTING AMONG REGISTERED VOTERS IN CPS SURVEYS, 2002 AND 2006

2002

	Without Voter ID Laws		*With Voter ID Laws*			
	coefficient	*ordinary std. error*	*coefficient*	*ordinary std. error*	*robust std. error*	*clustered std. error*
Age	0.0534	0.0032	0.0534	0.0032	0.0038	0.0052
Age-squared	-0.0002	0.0000	-0.0002	0.0000	0.0000	0.0001
Female	-0.0523	0.0178	-0.0520	0.0178	0.0207	0.0199
Married	0.2740	0.0201	0.2744	0.0201	0.0235	0.0271
White	-0.1756	0.0255	-0.1827	0.0255	0.0322	0.0657
No HS Degree	-1.1981	0.0350	-1.1980	0.0350	0.0416	0.0634

HS Degree only	-0.5405	0.0216	-0.5394	0.0216	0.0248	0.0383
Income[FN] [FNa]	0.0469	0.0030	0.0468	0.0030	0.0036	0.0057
Income missing	0.4878	0.0396	0.4886	0.0396	0.0471	0.0691
Voter ID Laws			-0.0383	0.0062	0.0071	0.0312
(0-7 Scale)						
Intercept	-1.2320	0.0824	-1.1422	0.0837	0.0837	0.1217
McKelvay-Zavoina Pseudo R^2		0.328		0.329		
N		67,174				

2006

	Without Voter ID Laws		*With Voter ID Laws*			
	coefficient	*ordinary std. error*	*coefficient*	*ordinary std. error*	*robust std. error*	*clustered std. error*
Age	0.0584	0.0031	0.0586	0.0031	0.0038	0.0045
Age-squared	-0.0003	0.0000	-0.0003	0.0000	0.0000	0.0000

Female	-0.0358	0.0186	-0.0355	0.0186	0.0214	0.0239
Married	0.1527	0.0208	0.1537	0.0208	0.0242	0.0374
White	-0.0757	0.0257	-0.0794	0.0257	0.0316	0.0687
No HS Degree	-1.1978	0.0369	-1.1941	0.0369	0.0432	0.0608
HS Degree only	-0.5886	0.0224	-0.5868	0.0224	0.0258	0.0357
Income[FN] [FNa]	0.0538	0.0030	0.0536	0.0030	0.0035	0.0047
Income missing	0.5972	0.0407	0.5977	0.0407	0.0479	0.0828
Voter ID Laws (0-7 Scale)			-0.0345	0.0049	0.0058	0.0309
Intercept	-1.2683	0.0821	-1.1677	0.0832	0.0997	0.1550
McKelvay-Zavoina Pseudo R^2		0.315		0.317		
N		64,251				

FNa. Income is measured as the income intervals in the CPS codebook. McKelvay-Zavoina Pseudo-R2 is the estimated ratio of the explained variance (of the prediction equation) to the variance of the underlying latent dependent variable.

Our motivation for the individual-level controls is not so much that individual characteristics are a source of spurious relationship. That is, we assume that any change in individual-level motivation to vote between the two elections will be

roughly constant across demographic categories and unrelated to state changes in voter identification laws. Rather, the chief advantage of constructing the residual turnout rate is to ensure as much as possible that the observed change in state turnout (among registrants) is a function of state-level factors alone and not 2002 to 2006 differences in the demographic composition of the CPS's sampling of the states. The state residual turnout levels for 2002 and 2006 differ considerably***92** because states differ in their turnout levels apart from their demographic composition. Our task would be simplified if state-level changes in turnout were uniform across states apart from those caused by changes in voter identification laws. In actuality, state voting rates change from one election to the next for a variety of reasons. Such changes increase the size of the disturbance term in the regression equation we use to predict residual turnout change caused by change in the voter identification law.

Because certain types of individuals may be particularly inhibited by voter identification laws, we also performed subgroup analysis. We analyzed observed and demography-adjusted turnout levels for three subgroups: college educated with B.A. degrees or higher (who presumably are little affected), those with no more than a high school diploma, and grade school educated without a high school degree (who presumably are most subject to any deterrent effects of voter ID legislation). We also separately analyze respondents scoring low on a multi-item index of presumed vulnerability based on demographic characteristics (details not shown).

A WRONG PATH

We could have proceeded, misguidedly, by pursuing a cross-sectional analysis. We might even have been tempted into using our 64,000-plus respondents as our units rather than our 51 states. It is worthwhile considering how we would have been led astray.

Consider again the individual-level equations of Table 1. The second set of coefficients for each year (labeled “With Voter ID Laws”) adds year-specific state scores on the 0-7 index of voter ID legislation to supplement the existing variables. For both 2002 and 2006, the coefficient for voter identification laws is negative, as theory would suggest. Unadjusted, the standard errors for net change in legislation produce absolute *t*-values of greater than 6. In other words voter ID legislation is a “significant” negative predictor of turnout at better than the .001 confidence level. But even apart from important and obvious endogeneity concerns that arise (does the negative coefficient arise because states with less participatory cultures pass strict laws?), we must recognize that the reported significance level assumes the relevant degrees of freedom based on 64,000-plus individuals rather than based on a modest set of 51 states. Table 1 shows that if we employ “robust” standard errors, as do Vercellotti and Anderson (2006) and Mulhausen and Sikich (2007), we produce slightly more conservative estimates of significance for voter identification laws. But the robust standard errors correct only for heteroskedasticity, which is not the main problem. The whole approach, even with robust standard errors, is the wrong solution for dealing with our state-level policy variable, as the standard errors are still seriously deflated compared to what they should be. Table 1 also reports a third version of the standard errors, clustered by states, that corrects the problem. The result is that individual-level standard errors take into account within-state variance. More relevantly, the standard error for the clustered variable (voter ID laws) is now based on the number of states, not respondents. With the standard error for laws now expanded by a factor of about 7, we see that state laws are not close to statistically significant. The clustered standard errors are barely larger than the coefficients themselves. [FN4]

The intuition for this result may not be immediately obvious. If state turnout levels varied almost entirely based on the changes in voter ID requirements (plus the individual characteristics in the equation), there would be no problem. But of course that is not the case. Aggregated to the state level, the correlation between the predicted vote (from individual characteristics plus voter ID law) and the actual vote is a mere .39 for 2002 and .38 for 2006. States vary in their rate of voting participation ***93** largely for reasons that are unmeasured by demographic variables in the Current Population Survey. [FN5]

DIFFERENCE IN DIFFERENCES

Working with change over time alleviates the endogeneity problem. Presumably states do not rapidly change their culture of participation because of a change in the law or for other reasons. Potentially, working with change also increases the efficiency of the estimates. The reason is that although states vary in their un-modeled influences on turnout, they presumably vary

little in their *change* in un-modeled influences on turnout. High-turnout states in 2002, for instance presumably are high-turnout states in 2006. By this reasoning, there should be less unexplained variance when modeling change in the vote over time rather than cross-sectional turnout. At the same time, since turnout estimates contain sampling error, this source of error will double when examining change scores. [FN6]

The dependent variables for the difference-in-differences analysis are the change in the turnout rate between 2002 and 2006 among the entirety of states' registered voters, as well as among more demographically select groups. We analyze state change both ignoring and controlling for the effects of demography on turnout within the state CPS sample. The variances of the various potential dependent variables are shown in Table 2. Change scores have less variance, but only slightly so, than levels of turnout. Adjusting the state samples for sample demography also offers a slight reduction of the variance to be explained. The less the variance, the less the uncontrolled variance to be explained. [FN7]

Still, the gains from the lesser variance turn out to be slight. One might be surprised that adjusting for individual characteristics of the state samples contributes so little. After all, the usual suspects—age, education, income, race, gender, marital status—all matter at the individual level. But many of them, especially gender, marital status, and age, only vary marginally at best when accounting for state-to-state differences. [FN8]

Table 3 presents the coefficients and standard errors for the effect of change in voter ID legislation utilizing the difference-in-differences analysis. Change is measured two ways, as net change in the state score, 2002-2006, and dichotomously as the presence or absence of any increase in severity. The results are shown for all voters plus three segments based on education. Results are presented with and without the adjustment for sample demographics.

Some of the results are displayed graphically in Figures 1-6. In appearance, these graphs support the hypothesis of a depressing effect on turnout. They show scatterplots overlaid with regression lines. Figures 1 and 2 show the pattern when generalizing to all registered voters. We see that whether using observed (Fig. 1) or adjusted (Fig. 2) turnout estimates, as a state shifts from low to high scores on the voter ID law scale, expected turnout declines by about two percent. This pattern is in the range one might expect and seemingly supports the suppression hypothesis.

The problem, however, is that these estimates are decidedly not significant. None of the estimates for all voters or even for the "target" non-high school educated group is close to being statistically significant. The rough pattern is that as laws become severe turnout declines at about the modest magnitude one might expect. The significance levels (in the .50 range) ***94** tell us that if the null hypothesis were true (no effect), the observed pattern could easily be a slight turnout decline with increasing law severity on the order of magnitude that is observed.

TABLE 2. STANDARD DEVIATIONS OF STATE VOTING RATES FROM CPS SURVEYS

	2002		*2006*		*2006 minus 2002*	
	Observed	*Adjusted*[FN] [FNc]	*Observed*	*Adjusted*[FN] [FNc]	*Observed*	*Adjusted*[FN] [FNc]
All	6.1	5.8	6.7	6.1	5.0	5.1
Grade School[FN] [FNa]	7.6	7.5	9.5	9.2	8.4	8.1

High School[FN [FNb]]	6.2	6.2	7.1	6.7	6.0	5.7
College Graduate	6.5	6.0	5.2	5.2	5.2	5.1

N = 51 (states plus D.C.)

FNa. No High School degree.
FNb. High School degree but no B.A.
FNc. Adjusted standard deviations equal the standard deviations of the deviation of observed state turnout from expected state turnout based on respondent individual characteristics from Table 1.

One further test might offer hope of a better resolution. We observe that change in legislation has as close to zero "effect" as possible for the college educated, especially when adjusted for individual characteristics. This is consistent with theory, since college-educated citizens should not be easily deterred by voter ID laws. We could perform a difference-in-differences-in-differences analysis comparing the states' change among possibly vulnerable non-high school graduates compared to the change among the states' college educated. In other words, we ask whether an increase in voter ID severity reduces turnout among the non-high school educated more than among the college educated. The answer again is a pattern that is decidedly not significant. See Figures 3-5 for the data display.

TABLE 3. ESTIMATED EFFECTS OF VOTER ID LAWS ON TURNOUT AMONG REGISTERED VOTERS

Independent Variable = Net Change Score in Voter ID Law

	Dependent Variable = Change in Observed Voting Rate				*Dependent Variable = Change in Adjusted Voting Rate*			
	coefficient	*std. err.*	*p-value*	R^2	*coefficient*	*std. err.*	*p-value*	R^2
All	-.41	.44	.34	-.0017	-.33	.44	.46	-.0087
Grade School[FN [FNa]]	-.43	.72	.61	-.0130	-.29	.70	.69	-.0169
High	-.54	.52	.30	.0020	-.49	.49	.32	.0002

School[FN] [FNb]								
College Graduate	-.10	.45	.82	-.0193	.03	.44	.95	-.0203
Grade School	-.49	.67	.47	-.0091	-.32	.63	.62	-.0150
minus College								

Independent Variable = Presence or Absence of Increase in Voter ID Law (0 or 1)

	coeffi-cient	*std. err.*	*p-value*	R^2	*coeffi-cient*	*std. err.*	*p-value*	R^2
All	-1.8	1.5	.25	.0072	-1.5	1.5	.34	-.0014
Grade School[FN] [FNa]	-2.0	2.5	.45	-.0081	-1.5	2.5	.56	-.0131
High School[FN] [FNb]	-1.8	1.8	.31	.0011	-1.8	1.7	.32	.0003
College Graduate	-1.7	1.6	.29	.0025	-1.2	1.5	.46	-.0009
Grade School	-3.1	2.3	.90	-.0200	-3.1	2.2	.89	-.0200
minus								

College

N = 51 (states plus D.C.)

FNa. No High School degree.
FNb. High School degree but no B.A.
FNc. Adjusted data represent the differences between observed stae observations and the turnout expected based on respondent individual characteristics from Table 1.

***95 FIG. 1.** Change in voter turnout by change in voter ID laws; all cases, observed state turnout data.

TABULAR OR GRAPHIC MATERIAL SET FORTH AT THIS POINT IS NOT DISPLAYABLE

Figure 6 further confirms these findings. It shows turnout for voters likely to be the most vulnerable to strict ID laws, measured by an additive scale combining minority status, low income, low education, and age. The scale identifies "voter ID vulnerability" based on a score of 3 or 4 on our index adding one point each for "nonwhite," "lowest 20 percentile income level," "no high school diploma," and "under 25 or over 64." The effect is bigger than usual, a "loss" of over one point of turnout per point of law severity. But, again, the findings are not statistically significant. The variance by state is high because, as for the lowest educated group, our sample size is small.

FIG. 2. Change in voter turnout by change in voter ID laws; all cases, adjusted for demographic characteristics of individual CPS respondents.

TABULAR OR GRAPHIC MATERIAL SET FORTH AT THIS POINT IS NOT DISPLAYABLE

***96 FIG. 3.** Change in voter turnout by change in voter ID laws; non-high school graduates, adjusted for demographic characteristics of individual CPS respondents.

TABULAR OR GRAPHIC MATERIAL SET FORTH AT THIS POINT IS NOT DISPLAYABLE

DISCUSSION

On the one hand we can observe average turnout "effects" that mimic the plausible complaint of critics. The average estimate is that going from lax to severe voter ID requirements is associated with a couple of percentage points less in the voting rate, as found by the Vercellotti and Anderson study (2006), Muhlhausen and Sikich (2007), and Alvarez et al. (2008). Moreover, this decline is found mainly among the least educated. But the lesson here is that ***97** this estimate is statistically inconclusive. The pattern as described is not close to statistical significance. This is true even if we control for the demographic characteristics of the respondents in the CPS state surveys. We could obtain the slight state differences that are consistent with theory by chance even if the true impact of voter identification laws on turnout is a zero effect.

FIG. 4. Change in voter turnout by change in voter ID laws; college graduates, adjusted for demographic characteristics of individual CPS respondents.

TABULAR OR GRAPHIC MATERIAL SET FORTH AT THIS POINT IS NOT DISPLAYABLE

FIG. 5. Change in voter turnout by change in voter ID laws; college educated minus non-high school graduates, difference in

differences in differences analysis.

TABULAR OR GRAPHIC MATERIAL SET FORTH AT THIS POINT IS NOT DISPLAYABLE

We obtain this inconclusive result because state turnout varies considerably apart from the variables of our analysis. One can see this ***98** from Figures 1-6. The observations are considerably dispersed around the regression line. Our imagination might tell us that shifts in voter turnout, especially among registered voters, vary little from state to state. If that were the case, the observations would cluster around the regression lines and we would be obtaining estimates of statistically significant voter ID effects.

FIG. 6. Change in voter turnout by change in voter ID laws; CPS respondents scoring high on index of voter ID vulnerability.

TABULAR OR GRAPHIC MATERIAL SET FORTH AT THIS POINT IS NOT DISPLAYABLE

Our conclusions are in contrast to the claims of Alvarez et al. (2008) in their analysis of CPS voter participation data. We obtain estimated "effects" of similar magnitude to theirs. Yet we differ in our reports of the precision of our estimates. Whereas we see our results as decidedly non-significant, Alvarez et al. report tight ranges to their coefficients that suggest otherwise. We stand by our interpretation that the evidence is far too shaky to stake a claim of discovery. [FN9]

The moral is simple. We should be wary of claims—from all sides of the controversy—regarding turnout effects from voter ID laws based on current CPS data. The effects may be there. By all tests there is nothing to suggest otherwise. But the data are not up to the task of making a compelling statistical argument.

CONCLUSIONS

It should be evident that our sympathies lie with the plaintiffs in the voter ID cases. Yet we see the existing science regarding vote suppression as incomplete and inconclusive. This is not because of any reason to doubt the suppression effect but rather because the data that have been analyzed to date do not allow a conclusive test.

What can be done to boost the empirical analysis of the problem? Additional elections and additional states enforcing strict voter ID laws will provide more and better data. Beyond that, we suggest a more detailed analysis not of survey turnout data, but of aggregate data within and between states. Here is one difference-in-differences-in-differences design: suppose we observe a decline in the voting rate in disadvantaged precincts of a strict-enforcement state such as Indiana relative to the voting rate of advantaged precincts within the state. This would be evidence that the poor are voting less relative to the rich, but is this because of the voter ID law? A test would be whether the decline is present only in states with new voter ID laws and not in states without them. And then, even if there is an effect, the test will work only if changes in the rich-poor voting gap are rare in the absence of newly enacted voter ID laws. State differences in respondent turnout and change in turnout are too vast for the voter ID law effect to be measured by the CPS with sufficient precision. Conceivably this problem can be alleviated by using within-state aggregate voting returns, which, whatever their demerits, are free of the noise from survey sampling error.

A more modest but still promising approach is to fall back on surveys of who has or does not have the kinds of identity documents mandated in recent voter identification legislation. Turnout questions aside, we don't see why, for now, a straightforward approach isn't enough to raise concerns about a disparate impact of voter ID laws. Recent research of this kind strongly suggests that strict voter ID laws will negatively affect certain voters, including minorities, at least in the short-run (Pawrasarat 2005; Brennan Center for Justice 2006; Barreto, Nuño and Sanchez 2007a; 2007b; Pastor et al. 2008; Hood and Bullock 2008). Until we have more experience with restrictive voter ID laws that are already on the books and, therefore, more data to analyze, survey findings and database matching showing thousands, perhaps millions of citizens lacking government-issued photo ID should raise red flags for policymakers and voting rights advocates alike that these laws could prevent eligible voters from voting.

***99** DATA APPENDIX

		Voter ID Law[FN [FNa]]		*2002 Voting Rate*[FN [FNb]]		*2006 Voting Rate*[FN [FNb]]	
	State	*2002*	*2006*	*Observed*	*Adjusted*[FN [FNc]]	*Observed*	*Adjusted*[FN [FNc]]
1	AL	1	4	67.5	.3	68.0	-.9
2	AK	4	4	75.8	7.4	75.5	4.7
3	AZ	1	4	72.3	1.5	75.6	3.4
4	AR	3	4	72.6	4.4	69.9	-.1
5	CA	1	1	69.4	-.6	77.2	4.1
6	CO	1	4	75.0	5.0	76.9	3.9
7	CT	4	4	67.5	-4.4	74.7	.6
8	DE	5	4	65.8	-4.5	68.4	-3.9
9	DC	1	1	70.1	1.3	68.5	-2.9
10	FL	5	7	73.1	3.1	68.8	-4.4
11	GA	4	4	65.0	-1.7	68.5	-1.6
12	HA	3	6	85.4	10.7	79.5	4.0
13	ID	1	1	74.9	5.7	79.6	8.1

14	IL	2	2	69.4	-.1	69.6	-2.4
15	IN	1	7	65.5	-4.0	70.1	-1.1
16	IA	1	1	70.4	1.9	71.7	.5
17	KS	1	1	72.7	1.8	71.0	-2.0
18	KY	4	4	67.6	.6	68.3	-.4
19	LA	4	6	67.0	-.1	57.1	-12.6
20	ME	0	0	71.5	2.7	74.1	3.3
21	MD	1	4	76.8	4.4	79.9	6.3
22	MA	3	3	73.1	2.1	77.3	3.5
23	MI	1	1	69.6	1.5	78.8	8.3
24	MN	1	1	84.8	16.3	83.8	11.2
25	MS	1	1	61.0	-5.7	62.0	-5.8
26	MO	4	4	71.5	2.7	74.1	3.6
27	MT	1	4	77.6	7.7	85.4	14.8
28	NE	1	1	65.1	-4.0	74.9	2.8

29	NV	2	2	75.4	4.5	76.7	3.3
30	NH	0	0	77.1	5.8	70.4	-3.9
31	NJ	2	2	65.8	-6.3	70.7	-4.6
32	NM	1	4	75.2	7.4	78.1	6.2
33	NY	2	2	65.5	-4.2	67.9	-4.8
34	NC	0	0	69.2	-.7	59.2	-11.5
35	ND	0	4	68.7	2.1	66.3	-2.5
36	OH	2	4	66.5	-2.5	75.3	5.2
37	OK	1	1	72.5	3.6	67.4	-2.7
38	OR	2	2	79.1	9.7	83.9	11.5
39	PA	2	2	68.1	-1.7	73.8	1.7
40	RI	0	0	75.1	4.5	81.2	8.9
41	SC	5	4	68.5	-.3	70.1	-.3
42	SD	0	6	87.4	19.1	81.2	11.2
43	TN	2	4	73.3	5.0	72.1	1.5

44	TX	4	4	61.4	-5.5	58.2	-11.8
45	UT	0	0	68.0	-.4	65.8	-5.3
46	VT	0	0	75.0	4.8	79.8	6.8
47	VA	4	4	59.0	-12.1	72.3	-.8
48	WA	1	4	72.2	2.1	77.3	4.2
49	WV	2	2	61.2	-6.9	59.9	-8.9
50	WI	3	3	72.8	3.0	80.9	9.4
51	WY	0	0	82.6	13.9	79.0	7.8

FNa. Scale constructed by Alvarez, Bailey, and Katz (2008).
FNb. As a percentage of self-reported registered voters in CPS surveys.
FNc. Adjusted state means are mean deviations of observed turnout from in the CPS survey samples from turnout predicted by individual characteristics. See Table 1 for predictor variables.

***100 REFERENCES**

Alvarez, R. Michael, Delia Bailey, and Jonathan N. Katz. 2008. "The Effect of Voter Identification Laws on Turnout." California Institute of Technology Social Science Working Paper 1267R. January. Available at: ‹http:// papers.ssrn.com/sol3/papers.cfm?abstract_id=1084598›.

Ansolabehere, Stephen and Nathaniel Persily. 2008. "Vote Fraud in the Eye of the Beholder: The Role of Public Opinion in the Challenge to Voter Identification Requirements." *Harvard Law Review*. 121(7): 1738-74.

Barreto, Matt A., Stephen A. Nuño, and Gabriel R. Sanchez. 2007a. "Voter ID Requirements and the Disenfranchisements of Latino, Black and Asian Voters." Paper prepared for the Annual Meeting of the American Political Science Association, Chicago, Illinois. 30 August-2 September.

_____. 2007b. "The Disproportionate Impact of Indiana Voter ID Requirements on the Electorate." Working Paper, Washington Institute for the Study of Ethnicity and Race. 8 November. Available at ‹http:// depts.washington.edu/uwiser/documents/Indiana_voter.pdf›.

Bertrand, Marrianne, Esther Duflo, and Senhil Mullainathan. 2004. "How Much Should We Trust Difference in Differences Estimates?" *Quarterly Journal of Economics*. 119(1): 249-275.

Brace, Kimball W. 2005. "Report on the Matching of Voter Registration and Driver's License Files in *Indiana Democratic Party et al. v. Todd Rokita et al*. Court Case." Submitted with Affidavit of Kimball W. Brace. Brief in Support of Motion for Summary Judgment. Exhibit 5. September.

Branton, Regina P. 2004. "Voting in Initiative Elections: Does the Context of Racial and Ethnic Diversity Matter?" *State Politics and Policy Quarterly*. 4(3): 294-317.

Brennan Center for Justice and New York University Law School. 2006. "Citizens Without Proof: A Survey of Americans' Possession of Documentary Proof of Citizenship and Photo Identification." Voting Rights & Elections Series. November. Available at ‹http://www.brennancenter.org/page/-/d/download_file_ 39242.pdf›.

Brief of Amici Curiae of Historians and Other Scholars in Support of Petitioners. *Crawford et al. v. Marion County Election Board et al*. Nos. 07-21, 07-25 (U.S., 2007).

Brief of State Respondents. *Crawford et al. v. Marion County Election Board et al*. Nos. 07-21, 07-25 (U.S., 2007).

Campbell, Donald T. and Juilian C. Stanley. 1966. *Experimental and Quasi-Experimental Designs for Research*. Chicago: Rand McNally.

Donald, Stephen G. and Kevin Lang. 2007. "Inference with Difference-in-Differences and Other Panel Data." *Review of Economics and Statistics*. 89(2): 221-233.

Eagleton Institute of Politics, Rutgers, The State University of New Jersey and The Moritz College of Law, The Ohio State University. 2006. "Report to the U.S. Election Commission on Best Practices to Improve Voter Identification Requirements, Pursuant to Help America Vote Act of 2002, Public Law 107-252." 28 June. Available at ‹http://www.eac.gov/Freedom%20of%CC20Information%
20Act/docs/report-to-the-u-s-eac-on-best-practices-to-improve-provisio.pdf/attachment_download/file›.

Electionline.org. 2006. *Election Reform: What's Changed, What Hasn't and Why, 2000-2006*. February. Available at ‹http:// www.pewtrusts.org/uploadedFiles/wwwpewtrustsorg/Reports/Election_ reform/electionline_022006.pdf›.

Gordon, Greg. 2008. "With No Photo IDs, Nuns Denied Ballots in Indiana." McClatchy Washington Bureau. 6 May.

Hastings, Deborah. 2008a. "Saint Mary's Nuns Told: No Photo IDs, No Vote." *South Bend Tribune*. 7 May.

_____. 2008b. "Indiana Nuns Lacking ID Denied at Poll by Fellow Sister." *Associated Press*. 6 May.

_____. 2008c. "Indiana's Primary Turnout High, Despite Photo ID Law." *Associated Press*. 8 May.

Hood, M.V. III and Charles S. Bullock, III. 2008. "Worth A Thousand Words? An Analysis of Georgia's Voter Identification Statute." *American Politics Research*. 36(4): 555-579.

Indiana Secretary of State Todd Rokita. 2008. "2008 Post Election Update." Press Release. 7 May. Available at ‹http://in.gov/sos/press/2008/05_07_ 08postelection%20update.pdf›.

"Indiana Voter ID Law Disenfranchised Some." 2008. Editorial. *The (Muncie, IN) Star Press*. 9 May, 4A.

Jacobs, Lawrence R. and Melanie Burns. 2008. "The Big Mobilization: Increased Voter Registration in 2008." Report Prepared by the Center for the Study of Politics and Government, Humphrey Institute of Public Affair, University of Minnesota. 5 May. Available at ‹http:// www.politicsandgovernance.org/reports/2008/The_Big_Mobilization.pdf›.

Kousser, J. Morgan. 1974. *The Shaping of Southern Politics: Suffrage Restriction and the Establishment of the One-Party South, 1880-1910*. New Haven: Yale University Press.

Logan, John R. and Jennifer Darrah. 2008. "The Suppressive Effects of Voter ID Requirements on Naturalization and Political Participation." Report of the American Communities Project, Brown University. 2 January. Available at ‹ http://www.s4.brown.edu/voterid/Voter%20id%CC20report%20final.pdf›.

Lott, John. 2006. "Evidence of Voter Fraud and the Impact That Regulations to Reduce Fraud Have on Voter Particpation Rates." Working Paper. University of Maryland. Available at ‹http://papers.ssrn.com/sol3/papers.cfm?abstract_ id=925611›.

Martelle, Scott. 2008. "Nuns Are Turned Away From Indiana Polls Under Voter ID Law." *Los Angeles Times*. 7 May.

Milyo, Jeffrey. 2007. "The Effects of Photographic Identification on Voter Turnout in Indiana: A County-Level Analysis. Report 10-2007." Institute of Public Policy, University of Missouri, Columbia. November. Available at ‹ http://truman.missouri.edu/uploads/Publications/Report%2010-2007.pdf›.

***101** Minnite, Lori and David Callahan. 2003. "Securing the Vote: An Analysis of Voter Fraud." New York: Demos, A Network for Ideas & Action.

Minnite, Lorraine. 2007a. "The Politics of Voter Fraud." A Report to Project Vote. March.

_____. 2007b. "Election Day Registration: A Study of Voter Fraud Allegations and Findings on Voter Roll Security—A Preliminary Report." New York: Demos, A Network for Ideas & Action.

Moulton, Brent R. 1986. "Random Group Effects and the Precision of Regression Estimates." *Journal of Econometrics*. 32(3): 385-397.

_____. 1990. "An Illustration of a Pitfall in Estimating the Effects of Aggregate Variables in Micro Units." *Review of Economics and Statistics*. 72(2): 334-338.

Muhlhausen, David B. and Keri Weber Sikich. 2007. "New Analysis Shows Voter Identification Laws Do Not Reduce Turnout." A Report of The Heritage Center for Data Analysis. CDA07-04. 10 September.

Mycoff, Jason D., Michael W. Wagner, and David C. Wilson. 2007. "The Effect of Voter Identification Laws on Aggregate and Individual Level Turnout." Paper prepared for the Annual Meeting of the American Political Science Association, Chicago, Illinois. 30 August-2 September.

Pastor, Robert, Robert Santos, Alison Prevost, and Vassia Gueorguieva. 2008. "Voter IDs Are Not the Problem: A Survey of Three States." Center for Democracy and Election Management, American University. 9 January. Available at ‹http:// www.american.edu/ia/cdem/pdfs/VoterIDFinalReport1-9-08.pdf›.

Pawasarat, John. 2005. "The Driver License Status of the Voting Age Population in Wisconsin." Employment and Training

Institute, University of Wisconsin-Milwaukee. June. Available at ‹http:// www4.uwm.edu/eti/barriers/DriversLicense.pdf›.

Piven, Frances Fox and Richard A. Cloward. 2000. *Why Americans Still Don't Vote and Why Politicians Want It That Way*. New York: Beacon Press.

Primo, David M., Matthew I. Jacobsmeier, and Jeffrey Milyo. 2007. “Estimating the Impact of State Policies and Institutions with Mixed-Level Data.” *State Politics and Policy Quarterly*. 7(4): 446-459.

Project Vote. 2007. “Restrictive Voter ID Requirements. Issues in Election Administration.” Policy Brief Number 8. 23 March. Available at ‹http:// projectvote.org/fileadmin/ProjectVote/Policy_Briefs/Project_Vote_Policy_Brief_8_Voter_ID.pdf›.

Rogers, William. 1993. “Regression Standard Errors in Clustered Samples.” *Stata Technical Bulletin* 13: 19-23.

Vercelloti, Timothy and David Anderson. 2006. “Protecting the Franchise, or Restricting It? The Effects of Voter ID Requirements on Turnout.” Paper prepared for the Annual Meeting of the American Political Science Association, Philadelphia, Pennsylvania. 31 August- 3 September.

“Voter Registration Numbers Increase By the Thousands.” 2008. *(Indiana) Carroll County Comet*. 16 April, 5A.

Wooldridge, Jeffrey M. 2003. “Cluster-Sample Methods in Applied Econometrics.” *American Economic Review*. 93(2): 133-138.

Address correspondence to:

Dr. Lorraine C. Minnite

Department of Political Science

Barnard College, Columbia University

3009 Broadway

New York City, NY 10027

E-mail: lcm25@columbia.edu

[FNa1]. Robert S. Erikson is Professor of Political Science at Columbia University. Lorraine C. Minnite is Assistant Professor of Political Science at Barnard College. The authors would like to thank the anonymous reviewers for the *Election Law Journal*, Shigeo Hirano, and Kelly T. Rader for critical comments on earlier versions of this article, and Vanessa Perez for research assistance.

[FN1]. *Crawford v. Marion County Election Board*, 128 S.Ct. 1610 (2008).

[FN2]. For findings strongly suggesting that incidents of voter fraud are rare in American elections today, see Minnite and Callahan (2003) and Minnite (2007a; 2007b).

[FN3]. The classic statement is by Moulton (1986, 1990). See also Donald and Lang (2007).

[FN4]. There are a few minor observations from Table 1 worth noting: almost always, the individual characteristics pass the usual threshold of statistical significance, as their *t*-ratio of coefficient to standard error generally exceeds 1.96. Gender and to a lesser extent, race, are the exceptions. We also note that adding state laws to the equations adds only minimally to the underlying explained variance. This should be no surprise. And the coefficients for the individual-level variables are virtually unaffected by adding state laws. This too should not be a surprise.

[FN5]. The clustered standard error adjusts for the clustering of the dependent variable at the state-level as well as shifting the relevant *N* from the number of individuals to the number of states. The standard error for voter identification laws approximates the standard deviation for the aggregate equation where the state-level mean log of the odds of voting is accounted for by the score of the voter identification law.

[FN6]. The sampling variance of a difference between two independent samples (e.g., states in 2002 and 2006) will equal the addition of the sampling variance for each sample separately.

[FN7]. The cross-sectional variance represents sampling variance plus true variance in state effects. The over-time (2002 to 2006) variance represents the doubled sampling variance (see note 6) plus the variance of any state-level effects.

[FN8]. The state samples are sufficiently large that adjusting for demographic characteristics of the state samples (analogous to pollsters post-stratifying their samples by demography) offers little improvement to the state voting rate estimates. For these reasons the gain from residualizing is modest.

[FN9]. Alvarez et al. offer few details regarding the nuts and bolts of their Bayesian methodology applied to the problem. The challenge for them is to show reasons for statistical confidence where in our view none exist.

8 Election L.J. 85

END OF DOCUMENT